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(8)

SUCCESS AND FAILURE OF  
ELECTROLYSIS IN URETHRAL STRICTURES,  
ESPECIALLY  
Dr. KEYES' METHOD REVIEWED.

BY

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**I**N the *New York Medical Journal* of October 6, 1888, an article entitled, "The Curability of Urethral Stricture by Electricity," "An investigation," by E. L. Keyes, M.D., appeared.

My first intention was to let the article pass without notice, awaiting to be summoned, as I know I shall be, to attend the requiem mass chanted over it, by the great scientists of the age, ere it is committed to the grave without hope of resurrection. But as I, in the article termed "the apostle of this creed," am charged with being "largely responsible in this country for modern electrolytic fervor as relating to urethral matters," "by most pretensions statements of results," which I am unable to sustain, when operating under the impartial eye of Dr. Keyes, it seems to me to be just to those surgeons upon whose "imagination" I have imposed, and to myself, that I should give a truthful statement of all that passed

between Dr. Keyes and myself regarding the cases which he has in his paper, mentioned my name in connection with, as well as be permitted to comment upon the results stated by Dr. Keyes in his tabulated report. I therefore divide my paper in two parts: Part I referring to cases VIII and IX, and Part II, to the remainder of Dr. Keyes' article.

#### PART I.

It is worthy of note that no important advance in science has ever been made without determined opposition from those who, from motives best known to themselves, are unwilling to admit that there can be better and easier paths than those they have been accustomed to travel. Galileo, on announcing that the earth revolved around the sun, was seized, condemned as a heretic, and under the penalty of death commanded to retract. Franklin was ridiculed when he asserted that he could draw lightning from the clouds and bottle it. Fulton was thought crazy when he said he could propel a ship by steam. Dr. Harvey was jeered at when he advanced his theory of the circulation of the blood. To-day all they said is accepted, and we wonder how people could have thought otherwise.

Investigations of scientific subjects are generally conducted for either the benefit of humanity at large, or the gratification of the investigator. In

the first instance they are, if intended to be of any value as to their results, conducted openly, honestly and impartially, the only object of the investigation being the ascertainment of the truth. In the second instance, the object of the investigation being only to satisfy some personal desire of the investigator, whether the investigation be conducted honestly or dishonestly of necessity depends entirely upon the purpose, whim, or caprice of the investigator.

In Dr. Keyes' article we find these words: "The case treated by Dr. Newman." "Case VIII." It is necessary that I should state, from Dr. Keyes' letter to me, exactly the circumstances under which I began the treatment of that case. January 10, 1888, Dr. Keyes wrote me, expressing his desire to investigate the electrolytic method of curing stricture of the urethra, and, among other things, said: "If the method yields as good results in the hands of others as you get, it should be popularized." I forthwith wrote Dr. Keyes that I would afford him such aid as I could in his investigation. Though I must confess that, judging from what Dr. Keyes has said in the past, I had little or no doubt but that the outcome of his investigation would be just what he has published or obtained in the manner that he has. On January 21, 1888, Dr. Keyes wrote

me: "I will ask to send one case to you. *I will examine it before it goes, and after you pronounce it cured* [Italics my own], if you will allow me to do so." Shortly thereafter the patient came to me accompanied by Dr. Keyes' assistant, together with a card from Dr. Keyes in these words: "Will you kindly treat this patient, who has double linear stricture, and let Dr. Fuller watch the process. I have his present condition written down." The patient was of the charity order; his residence was not vouchsafed to me, nor was I aware that the assistant was making secret notes of what I said and did; that even casual remarks were being quoted as scientific deductions, such as it was due \* \* \* to the "blizzard," with such omissions as he saw fit to make. Presumably Dr. Keyes was ignorant of all this, for I would not lightly charge him (especially when he "entered the investigation," as he says, "without bias," \* \* \* \* \* "and from no motive except that of attempting to throw light upon a subject which is usually shrouded in much mystery"), with knowledge of such a proceeding, nor do I intend to intimate that he was an accessory before or after to such a course, except in so far as he by his own paper admits.

On examination I found two strictures, one at four and one-half and the other at five and one-quarter inches

from the meatus, and that I found a cicatrix. On February 6, 1888, Dr. Keyes wrote: "Don't let us go on with the D. case (Case VIII) unless you are willing to abide by the result, for it will be loss of time. I will look up another which has not been cut." It will be observed that the investigation has at this point changed; it is no longer for personal information, but has become "star chamber" in its character. I, as a matter of course, declined to allow any case, which was not absolutely under my control, to be used as a test case, when I had hundreds to show, with indisputable proof as to results; nevertheless I did not decline to continue treatment of the patient; for had I, as I might, justifiably, doubtless I would have been accused of refusing to aid a brother practitioner in his attempt "to throw light upon a subject which is usually shrouded in much mystery." The treatment of the case proceeded till July 2, 1888, when Dr. Keyes withdrew the patient against my protest. On June 26, 1888, Dr. Keyes wrote me: "I shall sail for Europe next Tuesday morning, July 3. If, therefore, you could this week send me a report about D. (Case VIII), whom you are kindly treating for me, I shall be obliged to you, as I expect to be away all summer." On June 28, 1888, I wrote: "The present state of D. (Case VIII),

whom you sent to me for treatment for, as you expressed it, double linear stricture at four and one-half inches, etc.; at the present time that stricture has entirely disappeared. Nevertheless, as the patient is not well or cured, I found on examination, when D. first came to me, a second stricture at five and one-quarter inches from meatus, also contracture of the bladder, which propelled the sound, and sometimes caused such spasm that no instrument would pass beyond six inches. There is much irritation and discharge of mucus, pus and blood from the urethra. The second stricture is improved so that a No. 25 French will pass; but the ring constituting the stricture can be felt distinctly, and is not cured at the present time. For the spasm of the bladder he has not received any treatment, and the galvanic current certainly will never cure spasmodic actions of the bladder and urethra. I propose to treat D. for the latter trouble during the hot season, taking time, and report to you later in extenso. With," etc.

July 2, 1888, Dr. Keyes replied as follows: "I thank you for your letter, and particularly for your kindness in facilitating my study of the subject. *I have examined D.* [Italics my own.] You state that the only remaining trouble with him is deep urethral (or somewhere near the neck of the bladder) spasm, and that electricity is not com-



petent to overcome this condition. Therefore, while again thanking you, will you allow me to ask you not to give D. any further treatment, that I may have an opportunity to examine him again in the autumn in order to determine his ultimate condition." Why did Dr. Keyes write me this note, withdrawing the patient, on the second of July, when he knew I could neither see him personally nor reply by letter before he sailed for Europe; for he had written, "I shall sail for Europe next Tuesday morning, July 31," under date of June 26th.

July 18, 1888, I wrote Dr. Keyes: "In your favor mentioned, you change somewhat my plan in not letting me treat D. for the present, saying it will facilitate your study of the subject. As such I consider it, and your intention is perhaps best for that purpose. But I wish to call your attention again to my former letter, of the 28th of June, in which I said that the case is not cured; and, again, that I did not treat the case with its complications, but only the stricture as desired; and, further, as I had not the patient under my sole control, even did not know his residence or whereabouts, the result is not as favorable nor cured as I desired to see it."

Dr. Keyes, on Sept. 5, 1888, wrote: "I am just back from my trip. I have not yet seen D." He then stated that he

intended to say at a Congress in Washington all that he knew about electrolysis as a method of treating stricture: also telling when he was to read his paper; and further stating: "You have been most kind in assisting my study of the matter; but I regret to say that my final conclusion is not favorable. When I see D., I shall examine into his present condition, and add that to the paper." He concluded by stating that he thinks it hardly proper "to report your [my] case without asking you to be present," etc.

On Sept. 13, 1888, Dr. Keyes wrote: "I have examined D., and find recontraction—his stricture being at  $4\frac{1}{2}$  inches. I cannot find that any improvement has followed the use of electricity; and on close questioning I learn that his strictured area never had been cut [as he had allowed me to suppose]; but that the cutting was a meatotomy. Under these circumstances, I see no escape from reporting the case a failure; and since [there was no previous cutting], it seems to me to demonstrate that electricity has signally failed in removing organic stricture in this case. I am sorry it has so turned out; but so certainly it is."

Certainly I protested against Dr. Keyes' method of procedure; but without avail.

I have quoted these letters thus *in extenso*, and grouped them together:—

First. In order that the reader may judge whether Dr. Keyes kept his agreement with me: viz., "I will ask to send one case to you. *I will examine it before it goes, and after you pronounce it cured.*" [Italics my own.] It is to be presumed Dr. Keyes examined the patient before he sent him to me; for he states that he found, as I did, double *linear* [Italics my own] stricture at  $4\frac{1}{2}$  inches from the meatus. Next, did he wait for further examination till I "pronounced it cured?" His own letters show that he not only did not, but that he examined the patient when I said the stricture was not cured, and withdrew him from my care against my will. This is confessed in the surreptitious notes of his assistant. How are we to know how many other explorations of the urethra were made by any one during the progress of our treatment? Were I under a contract with Dr. Keyes to cure D. or pay a penalty for failure, could he recover the penalty under this proof? This is a test of whether I failed with electrolysis. I am informed, on submitting the correspondence to a legal gentleman, that he could not; because, as the man of law puts it, he has, by his own acts, put it beyond my power to carry out the contract." My position is, as the legal gentleman says, like to "where A agrees to build a house for B, and to have it finished in a given time, B to furnish

materials as A requires. B fails to furnish the materials as required by A; A fails to complete the work at the given time; B sues A for failing to fulfil his contract. He loses his case upon the ground that he has broken his contract, in that by his acts he prevented A from completing the work at the given time.

Second. That the reader may judge for himself whether Dr. Keyes conducted the investigation fairly, impartially, and with what end in view. Why was the residence of the patient concealed from me, and why were notes made of only so much of my procedure as his assistant saw fit? Would it not at least have been honorable to have made those notes in my presence, or at least with my knowledge? A few brief remarks as to those notes will satisfy any impartial reader of their utter worthlessness. For instance: May 23: "No. 25 electrode, five milliamperes, ten minutes; will not enter the bladder." This note does not state whether the electrode passed the strictures, but simply "will not enter the bladder." How does the gentleman know this? I was operating, not he. Did I try to enter the bladder with the electrode? Certainly I did not. It was wholly unnecessary. The note does not say that I tried to enter the bladder with the electrode; but it certainly leaves room for the reader to conjecture that

I did and failed. It is certainly an innuendo to that effect, without a scintilla of truth in it. On "April 17, Dr. Keyes tested the patient as to his condition to-day. He found that only 12 (F.) would pass easily; the stricture bleeds; the meatus takes 29 (F.)." This is waiting for examination till I pronounce the patient cured. On the next day (18th) the patient is presented to me, and on examination I refuse to apply electricity. (See note of 18th April.) At that time I did not know of Dr. Keyes' performance on the 17th, but finding a condition of the stricture for which I was unable to account, sent the patient away for a week. I have not gone into the details of the printed report of Case VIII, because it seems to me that any one, by comparing the correspondence between Dr. Keyes and myself with those notes, will find food for meditation, the result of which will not be unfavorable to your humble servant.

"Case IX."—E. S. E. Dr. Keyes presented this case to his readers in these words. "The patient I now present for your inspection is a lamentable instance of the harm that may be done by the use of electricity in the urethra in careless hands," and further on says the patient got rather worse than better under Dr. Newman's treatment. I desire to put a few questions in order to determine how Dr. Keyes arrived at

these conclusions. Had Dr. Keyes examined the patient before I began treatment, and if he had not, how is he to know in what condition the urethra was before I commenced, or does he rely on what the patient said? The truth of the matter is, that the patient came to me for treatment before he went to Dr. Keyes, viz., on June 2. On examination I found that a No. 9 Fr. catheter passed with great difficulty, and only over a filiform guide. My last note is—Electrolysis No. 17, Fr., egg shaped bulb, tunnelled electrode, was introduced over a filiform guide—the trouble is only from two to three inches, after which the electrode passed at once into the bladder. No guide is any longer necessary. At this point I declined to continue further treatment of the patient, for reasons which if necessary I will give, but certainly not as Dr. Keyes alleges I said, “knowing his not to be a suitable case.” If Dr. Keyes understood me to say so, he most certainly misunderstood me. On my declining to further treat, the patient turned to me and said he feared lest he should fall into the hands of some quack, and asked me to recommend some reputable genito-urinary surgeon. I replied, “go to Dr. Keyes.”

In the above condition and under the above circumstances, the patient came to Dr. Keyes. Was he worse or better? Is a urethra admitting a No. 17 Fr.

electrode worse or better than when from stricture it only admitted a No. 9 Fr.? Why has Dr. Keyes mentioned my name in connection with the case, when he admits that all the damage was done by a Brooklyn homœopath?

How did Dr. Keyes relieve this patient? He cut twice, once up to No. 40 Fr., and the second time to No. 44, "cutting through the sheath of the urethra; indeed cutting everything except the skin." Was that treatment judicious?

#### PART II.

Dr. Keyes' article condemns electrolysis, thereby my method of treating urethral strictures, etc. In defending myself my task would have been easier to have dealt with facts than with surmises.

Dr. Keyes, in convincing his readers that I am all wrong, begins his article with:

"A widespread belief exists in the community. This belief includes the three presumptions: (1.) That any one, by following rules, may use the method successfully. (2.) That electricity does no harm to the urethra. (3.) That stricture cured by electricity is dissipated by absorption, and the urethra remains permanently open."

Surely it is no fault of mine, nor do I see how I can be held responsible for what a community presumes and be-

lieves. I will subscribe to No. 3, as in my extensive experience I always found on re-examination that patients once cured of urethral strictures by electrolysis suffered no re-contraction, and such re-examinations were made after from one to eleven years, respectively, by myself, and confirmed by the family physician. But I never said that any one can operate by electrolysis successfully, and that electricity will do the urethra no harm. On the contrary, I have in all my writings insisted on certain qualifications of the operator, and shown why some operators must necessarily fail. Among others, I will quote from my articles (*Armamentarium*, etc.): "If the foregoing rules are observed by an *expert* in *surgery* and *electricity*, success must follow. This has been proven by a vast amount of clinical facts. Some are aggrieved to hear that to succeed it is necessary to understand electricity and the handling of the genito-urinary instruments. If operators who are unskilled or careless fail in their undertaking, it does not condemn an acknowledged good operator. If some gentlemen use too strong currents, or the positive instead of the negative pole, they make gross mistakes, must necessarily fail, destroy tissues and ruin their patients."<sup>1</sup>

"Now, in conclusion, electrolysis of urethral strictures must and will suc-

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<sup>1</sup> Is Electrolysis a Failure?



ceed in proper hands in every case that is intelligently and judiciously undertaken. The operation itself needs a clear head, a steady hand, fingers which both see and feel, patience and good discrimination in the application of the strength of current and length of sitting."

Similar phrases have been used by me, and certainly I have not said anything by which a competent medical man is led to believe that any one can use electrolysis successfully, or that electricity does no harm to the urethra. Even a layman will not believe so, as he occasionally reads that an electric wire has killed a man coming in contact with it, and since our Legislature has passed a law to use electricity instead of the gallows for capital punishment. In another sentence of Dr. Keyes' article I find my name mentioned first in connection with a case which concludes as follows: "The patient consulted Dr. Robert Newman as an authority upon urethral electrolysis, who assured him he had no stricture, but still proposed to remove his urethral symptoms by electricity. The patient returned to me unrelieved, declining further trial of electricity." I am utterly lost to know what the author of those lines wanted to prove thereby, or for what purpose he wrote. But I am told by others, that the unsophisticated reader may make his own inference.

that Newman proposed an electric treatment, which was not indicated; that he did treat accordingly, and the patient returned to Dr. K. unrelieved.

While I know of no such case or facts, still I have corresponded with Dr. Keyes about a patient where there were some features in common. I therefore give the facts from note-book. January 31, W. came to my office; never has had any venereal disease, but had slight discharges from urethra, particularly in the morning after having drunk champagne; complains of a nervous organization, which causes exhaustion at times; thinks he has stricture at  $2\frac{1}{2}$  inches for six months; has been treated. Is a friend of Dr. Keyes, who has used deep urethral injections, which caused a profuse discharge and cystitis. Four days ago Dr. Keyes passed a bougie about as follows: No. 28 F. passed both ways; No. 30 passed tightly at  $2\frac{1}{2}$ , and returning would not pass, when he removed the instrument forcibly, causing pain and discharge. Dr. E. G. attends to him for nervous exhaustion, and Dr. W. treated him with the endoscope.

*Examination.* — A small bougie a boue causes sensitiveness. There are no indurations, the walls of the urethra feel soft; in fact, almost softer than in a normal state. Sometimes a contraction suddenly appears, which just as suddenly subsides.

The slightest and most careful contact of the instrument with the mucous lining of the urethra causes a burning sensation. Patient feels nervous, and for further examination will call in a few days; says Dr. Keyes has sent him to me to be treated for urethral stricture by electrolysis, and he will have the electricity when he calls again. February 5, Electrode No. 28 F. egg shaped bulb passed easily the suspected stricture at  $2\frac{1}{2}$  inches; but at  $3\frac{1}{2}$  a slight contraction suddenly appeared, and patient complained of pain. While the electrode was attached to the battery, but without any electric current being present, the index being at zero, patient complained of the severest burning he ever had in his life, which he imagined was done by the electricity. However, after practical demonstration, he had to acknowledge that the burning he thought he felt was not caused by electricity, as at that time no electricity was present. (Perhaps he had heard that electrolysis would burn.) The instrument detected sudden contractions, which disappeared. The whole examination on both days, as well as the history of the case, showed that there was no stricture, and that the contractions were due to spasm. On parting, I said: "Tell Dr. Keyes I said you have no stricture." It will be seen from the above notes that I examined the patient only, but certainly did not treat him;

therefore, neither cure nor relief could be expected from me. From the correspondence it will be seen that Dr. Keyes wrote me on February 6, 1888: "I have constantly insisted that he had no urethral disease—more than a little deep congestion spasm, and a little neurosis \* \* \* I told him that you were the electrolytic head-centre; and that is why he calls on you, and not that I thought he had stricture." As this conflicts with my notes made as stated by the patient, it is only justice to concede that neither Dr. Keyes nor myself are responsible for statements of patients who may unintentionally make mistakes in medical narratives, especially when they are excitable and nervous. I have stated in my notes what I understood the patient said, and certainly we believe everything that Dr. Keyes says. Why Dr. Keyes, who treated the patient for a neurotic condition, and knew there was *no* stricture present, gave his consent for electrolytic treatment for an alleged stricture by his family physician, I certainly do not know, and therefore cannot explain. The foregoing explanations are hardly necessary, except to show how and to what extent I was connected with this case, and to correct an impression which might be drawn from Dr. Keyes' article: namely, that I had treated the patient with electricity without relief.

The next part of the article is a résumé of my rules in electricity, my statistics and conclusions, well given in abstract, from which is seen that I recommended electrolysis, and have reported undoubted successes. So have others. In opposition, Dr. Keyes relates his own experience in seven cases, six of which were decided failures, and which, created as he states, could hardly have resulted otherwise, and one a success. He assures his readers that our relations have been most amicable, and I am very glad to hear this, as some who read the article might consider, from Dr. Keyes' report about me, that they were otherwise, even hostile. For my part, I have done all I could to nourish those friendly feelings, and hope the amicable relations between Dr. Keyes and myself will always exist. I also acknowledge with pleasure the good service Dr. Keyes has done me, as, since his article appeared, my patients for electrolysis, sent me by physicians who have read Dr. Keyes' report, have largely increased. Some expert in electrolysis recently said: "Now, some failures of electrolysis have been reported by gentlemen who said they followed Newman's method in every particular. But the truth is that they only thought they used Newman's method, while in reality they did not, and that caused failures." Now, I regret exceedingly Dr. Keyes' failure, I wish he had succeeded, and am ready

to assist him in any way I can. But, notwithstanding my repeated invitations to call and see for himself, he never came near me. Why he failed, I cannot say exactly, but will try to review his cases, and point out some facts which might have had some influence, and others from which I think some erroneous conclusions are drawn.

*Case I.* The patient is 74 years old; passes acid urine, somewhat turbid with pus, every two hours, with straining. Has been treated formerly for stricture by dilatation. At five inches is a tight stricture, admitting only a filiform whalebone. *January 30th.* Electrode, 16 Fr.; five milliamperes; three minutes; no effects.

In two other sèances, No. 16 electrode; no effect.

*Comment.* My rules say, use no larger instrument, at utmost, than three sizes larger than the stricture; in the beginning even a smaller number; as in this case only a filiform would be admitted, a small electrode of No. 9, and certainly not larger than a No. 11, would be indicated. The No. 16 was decidedly too large, and it is perfectly natural that "no effect" is reported. After 16 would not pass, why was 16 again used at the next seance? Why was not a smaller instrument tried? The former experience should have been taken into consideration. Why was he not treated also for the turbid urine with pus,

which was passed every two hours? Is this a case where the electrolysis must be held responsible for failures? enlarged the calibre of the urethra. But the report closes as follows: "In this case urethral fever, laying the patient up in bed, was produced by the electricity, with retention of urine and great pain and distress:

*Questions:*—What reason has the reporter to state that all the distress was caused by electricity? Is it not a fact that as a rule urethral fever and chill is produced by the handling of instruments in the urethra after any method of treatment? Do experts in electricity sometimes or often cause urethral fever? By what authority is it stated that electricity creates or even *can* create urethral fever?

Is not vesical tenesmus a common symptom of cystitis? Is there any tangible proof in this case that electrolysis must be called a failure?

To-day Mr. C., one of my patients from Texas, told me that he had been treated for stricture by surgeons of international repute, by urethrotomy, divulsion and dilatation, and that such treatment was followed often by chills and fever, and twice by blood-poisoning, which once laid him up six weeks; then adds that my applications of electrolysis never caused any unpleasant symptoms, even without having taken quinine.

*Case V.*—“Patient had to use a catheter in order to urinate, while a 19 Fr. blunt steel sound entered the bladder. Only one application of electrolysis was made. Retention followed electricity, and two days later perineal swelling and great local tenderness, making it difficult to walk.”

In this case the electricity nearly produced perineal abscess, and was abandoned.

*Comment.*—The history shows that the patient had retention before electricity was used. If a catheter No. 19 passed into the bladder, it certainly shows that the calibre of the urethra was large enough for voluntary micturition; but the patient could not urinate and had to use a catheter. Does that fact not prove that the retention was due to a pathological condition of the bladder, and not to the existence of a stricture?

Notwithstanding these facts the reporter accuses electricity, which was According to rules there could be no success. Whether some of the patient's symptoms were also due to an enlarged prostate is not stated.

In Case II the same mistake as commented on in Case I was made. There were two strictures present, admitting Nos. 19 and 23 Fr. respectively. Three numbers larger for the electrolysis would indicate a No. 22 and 26 respectively; as there are two strictures, and



a drop of blood has followed the withdrawal of the instrument, it would have been judicious to begin with 21 or 22; but certainly no larger instrument ought to have been used than a 25 Fr.; even that could not be expected to pass the spot, which admitted only a No. 19. The number 30, used in this instance, was four to five times too large. If I understand the case correctly, the first stricture admitted only a No. 23, and that five weeks ago. The largest instrument used in dilatation was No. 29 Fr. But it seems that my supposition and calculation are right when we find that at the next session "22 Fr. bulb, four milliamperes, four minutes," passed easily."

On April 23d, five different instruments were introduced at one sitting, and it seems with four instruments the electric current was used; such procedure is against any rule, and may cause a failure in almost every case. In one of my rules it is distinctly laid down that in one sitting only one instrument should be used.

Case III is such, that it is not fair to prove anything either way. On February 23d the report is entered, "mistaking the number, I try with some force (Italics are mine) to pass 21 Fr. soft (instead of 12); it will not go, but dilates the stricture. So that 16 Fr. bulb enters anterior stricture and allows me to use four milliamperes

“for six minutes, with a trace of blood.  
 “—Retention of urine for 24 hours  
 “after last sitting.” The question arises whether the retention of urine was caused by the force used, or by the electricity, or arose from other causes. Dr. Keyes says: in this case, therefore, retention of urine was produced by the treatment, in which I fully concur.

Case IV seems to show that the electric treatment did some good, and en- applied only once, of having caused retention, perincal swelling, of *nearly* producing perincal abscess. How are we able to ascertain a nearly produced perincal abscess? I presume the gentleman possesses a “divining rod,” which I “frankly” confess I have not.

In Case VI we find decided improvement after the use of electrolysis; in fact, a cure is admitted. But in the same breath the reporter almost apologizes for the patient's cure, and he then concludes that there had been no stricture, and if it was a stricture, it must have been a spasm of the bladder. What this all means I cannot understand. I understood Dr. Keyes to say that he investigated and tested electrolysis in strictures; therefore I expected him to select cases of organic strictures for his experiments. Certainly, he must know whether he has to deal with a stricture or spasmodic action. In the case under consideration he selected it, the electrolysis was tried, and after it

had benefitted he tells us, "Oh, that was no stricture!"

In Case VII the electrolysis is used only *once*, for seventeen minutes, and no after *effect* of a disagreeable sort from the electricity. Patient is frightened, and desires to enter a hospital.

I cannot see that this case, with so meagre a record, can be admitted as a proof, pro or con.

My connection with cases 8 and 9 has been fully given. In Chapter I, of the remaining seven cases in Dr. Keyes' statistical table, we find one case, No. 6, acknowledged as "cure permanent;" and the result of the other six cases summarily dismissed with one stereotyped word—"failure." Two of these cases had just *one* electric application (Cases V and VII).

Please observe my summary of the seven cases given above in detail:

3 cases were not treated according to my rules. In cases 1 and 2, too large electrodes were used in the beginning, and in case 3 force by mistake was used.

2 cases (5 and 7) had only one electrolytic application.

1 case (No. 4) is doubtful throughout, as it is not shown how urethral fever and chill was caused.

1 case (No. 6) is permanently cured.  
Total, 7.

This is the result in brief of Dr. Keyes' impartial, unbiased investigation of electrolysis in the treatment of ure-

thral strictures. This he concludes with the scientific remark, complimentary to all promoters of my method and reporters of successful cases, as follows :

‘My study of the subject and the experience it has brought me, digested with all the impartiality I possess, lead me to state that the allegation that electricity, however employed, is able to remove organic stricture radically, lacks the requirement of demonstration. The confidence of its advocates that it will radically cure organic fibrous stricture is, in my opinion, due either to the combined credulity of the patient and imagination of the surgeon, or to some special but fortuitous act of Providence, upon the co-operation of which, in the case of his own patients, the general practitioner cannot with any confidence rely.’”

‘Therefore, it follows that Dr. Keyes’ unfavorable report of seven cases stands as a fact for ever, like the Holy Bible; whereas the favorable reports of hundreds of cases by many medical men in different parts of the world are not worthy of belief, and are all “imagination.” I am not disposed to quarrel or to argue about such a conclusion. Let us be good-natured and agree. I agree perfectly with Dr. Keyes that he has failed in his seven cases with electrolysis, and I cannot help admiring his truth and honesty in publicly acknowledging his failures. Certainly, patients

who want to be treated by electrolysis will not go to Dr. Keyes, but to those who succeed with electrolysis. We know of many who do succeed, and about them I must say a word.

As a general rule, my method of electrolysis in urethral stricture has been accepted and practiced in almost all parts of the world. All possible and even impossible objections made have been answered and shown to be groundless. In an editorial of the *New England Medical Monthly*, December, 1887, a long list of successful operators was published. Some failures have been reported; generally good reasons existed why the attempts proved abortive (as stated above). In fact, it is wonderful that we do not hear of more failures, for, since the method has been more generally known and tried, some enterprising manufacturers have sold my electrodes by thousands.

Many purchasers had not the slightest idea how the instruments were to be used, and thought that a fine instrument case made the possessor an operator. So far, in every case of inquiry from any physician, I have always given information, personally or by letter, and with all possible courtesy made demonstrations. Nothing has been kept back; all is open to the profession. But it is impossible to go over the same ground with every physician in the United States; there must be a limit to it. Ex-

tensive correspondence about the matter, many cases treated for charity's sake, and demonstrations for 19 years past, have worn on my system and vitality; but it has been done cheerfully for the general good. I was always ready to work again, and by any test to convince the incredulous investigator. All I asked was that trials should be conducted fairly and impartially. While I am writing, an article just published in the *International Journal for Surgery and Antiseptics* comes to my view: "Why Electrolytic Treatment of Stricture does not succeed in all hands." In it, the author, Dr. T. C. H. Meier, has so clearly described, pictured, the causes of failure far better than I could, that I feel inclined to quote from him; but as I fear this paper is already long, so that it may tire the reader, I can do no better than to refer to it; it is worthy of perusal.

As so much has been said about failures, we must devote now a chapter to

#### SUCCESS IN ELECTROLYSIS.

I myself have practiced this method successfully for nineteen years, and off and on have honestly reported cases which were complete for such purpose. Hundreds of cases could not be used for publication, because the cases were too incomplete; many having been seen

only once in consultation, or operated on only once or a few times; many were lost sight of, and almost all of the charity cases in hospital and dispensary were even unknown by name. But the two series of one hundred cases each will stand on record, as I had previously reported cases in detail. The first 100 cases were selected particularly to show that no relapse had taken place; they were not consecutive cases, but collected from consecutive cases to meet the following conditions: The patients were to have been under treatment regularly for a reasonable time; they were to have been discharged as cured, or at least so improved that the patients were content with the result, and did not wish any further treatment or improvement; they were to be cases that were heard of afterwards, by reliable information, and mostly re-examined; a reasonable time having elapsed between the discharge when cured and the re-examination, which in these cases were respectively three to eleven years.

The proof of "no relapse" was that the same sized sound was used in the re-examination which passed the last time at the close of the treatment. In the report of my second 100 cases I have omitted under result the word, "cured," purposely, because there is a diversity of opinion as to the meaning of the word "cure." Some surgeons insist that there cannot be a cure ad-

mitted unless the urethra will admit No. 40 sound, and we have seen that the cutting has even been extended to a No. 44. My idea of a cure was, however, when patients were dismissed, or stopped treatment themselves because they felt comfortable and well, had a calibre of the urethra which enabled them to void freely a good sized stream, and, if wanted, could exercise sexual intercourse. Therefore, to suit my fastidious friends, I omitted the word "cure," and instead stated to what degree they were improved, and the size of the number to which the calibre of the urethra had been enlarged, etc. For the details of such results, I refer to the original papers, "Tabular Statistics of One Hundred Cases of Urethral Strictures Treated by Electrolysis," *New England Medical Monthly*, August, 1885, and "Synopsis of the Second One Hundred Cases of Urethral Stricture Treated by Electrolysis, with Cases," *Journal of the American Medical Association*, September 24 and October 1, 1887.

I am well sustained in my report of several hundred cases in a record of numerous cases by an array of physicians from all parts of the world, among whom I will mention, Drs. W. F. Hutchinson, T. F. Frank, David Prince, Jacob Butler, J. M. Glass, A. T. Douglass, D. O. Farrand, A. J. Wolff, J. B. Green, G. C. H. Meier, F. F. Dickman, R. J.



Nunn, T. F. Sanders, J. Craft, A. J. Wolff, Jr., W. C. Wile, Benson, Edw. J. Smith, R. W. St. Clair, J. H. Kellogg, G. W. D. Patterson, T. H. Burchard, I. Wolf, J. J. Berry, W. T. Belfield, Geo. E. Pitzer, C. A. Bryce, G. W. Overall in the United States; next comes Canada with Drs. C. R. Dickson, J. J. Cassidy, A. Laphorn Smith; and eminent surgeons in Great Britain; among them are W. E. Stevenson, W. Bruce Clark, Edwin Merton, T. J. Hayes, T. Swinford Edwards, etc. A recapitulation of this bibliography is also in a *New England Med. Monthly*, December, 1887. Suppose now that some cases have been reported prematurely as success in an overflow of enthusiasm—I do not say they have, I only suppose that it may have happened—suppose even that some have suffered a slight contraction in after years—there are certainly enough left by hundreds of cases, and if tabulated they probably will swell to thousands, which stand as a solid phalanx, as an unquestionable proof of success, which cannot be overcome by a few failures, even if such cases were fairly conducted according to established rules of surgery and electricity.

Successes of electrolysis in urethral strictures are on record in abundance, as stated above, and at the present time more documentary evidence is received, which I intended to incorporate here;

but the article will be too long, and I reserve most for another occasion.

I conclude by giving two letters, which have been sent to me by Dr. W. C. Wile; one his own statement of what he has seen about my treatment, and what he himself has done; and the other from an appreciative patient, who himself is a physician of standing, in active practice, and certainly knows what an organic stricture is, and how the treatment has affected him.

DANBURY, CONN., Nov. 24, 1888.

*My Dear Doctor:*—In reply to your courteous note of Nov. 24, I will state that I have seen cases successfully treated by you. Cases of stricture of the urethra which I have examined before and after treatment. I *know* organic stricture existed and that they were cured.

2. I have already treated about 50 cases myself, and all except spasmodic stricture, and those due to masturbation or of neurotic origin, have been either cured or so much relieved, that all the objectionable symptoms have disappeared and the patient passed a full stream with no inconvenience whatever, and gave up the treatment of his own accord. This I consider one of the greatest drawbacks to electrolysis, that the treatment is so painless, and the relief so sudden, the patients consider themselves well before they are,

Consequently, there is re-contraction, and the case is counted against the method by those who oppose it. Last Wednesday evening, before the Danbury Medical Society, I read a paper on this subject, challenging Dr. Keyes' conclusions, and demonstrating its utility by operating upon a patient of Dr. Brown's of this city, passing with the galvanic current in fifteen minutes a No. 20 electrode, where three weeks before, when Dr. Brown first brought him to me, we could with the utmost difficulty pass a filiform bougie—thus demonstrating by test, that in four sances I had dilated painlessly from almost nothing to a No. 20 French.

The patient was a hack driver, and, according to his own history, given that evening, before twenty or more physicians, he had *never had a moment's pain or inconvenience, and had attended to his duties right straight along.*

Now, if electrolysis can do no more good for this man than what had been already accomplished (and I know it will cure him), by what other methods, in the hands of even the *Electrolysis failurers*, Keyes himself, could so excellent a result be accomplished without an operation which would lay him up? I await an answer. I have on the table before me a letter from an eminent physician of Maryland, who came to me while in Philadelphia, for a stricture

which proved to be two inches and one-quarter long.

He is 67 years old, and it took me seven seances with a No. 11 French tunneled electrode, threaded on a fili-form bongie, the largest instrument that could be introduced at the time of my first seeing him. It took him three minutes and a quarter at that time to micturate. In reply to a letter from me immediately after the Keyes' explosion in Washington, he says :

“October 1, 1888.

“*Dear Doctor*—Your favor of — was received, and I was glad to hear from you. I have been thinking of writing to you for several months, but have been so on the go, that I neglected it. I have been gone most of the time since early summer, and arrived home a few weeks ago from an extended tour through Canada, the White Mountains, etc. I think (thanks to your skilful treatment) I am perfectly well of the stricture. Have not used the battery for four months; but pass No. 30 electrode every six weeks, without the slightest trouble, and without meeting with the least resistance anywhere in the urethral canal. If I did not know from previous experience, I could not tell in what part of the canal the stricture had been located. The stream is round and full; all irritability of the bladder is gone; and what is best of all, I have

not had an attack of the gout since the first seance, which is now over fifteen months; having never had over four months to elapse without an attack previous to the electrolysis, for the last three years. Of course, I cannot say positively that the removal of the stricture, which was hard and dense, and had existed since 1830, is the reason that I have been exempt from the gout; but I firmly believe it. My feet are not now tender at all, and I can wear shoes as tight as I could in my boyish days. I have not been travelling for my health at all; but for pleasure and the gratification of my better half. I never used more than seven cells; and, after twice using seven, never went beyond five. The last time I introduced the No. 30 electrode, which was a week ago, nine weeks had elapsed since its previous introduction, owing to being away; but I did not encounter the slightest trouble, pain or inconvenience on its introduction, and did it as quickly as you could introduce an ordinary catheter into a perfectly normal urethra. Of course I mean I did it without using the battery at all. When you consider how dense and hard and long standing the stricture was (over twenty-six years), and how the smallest electrode could not be passed, and even found it difficult to pass a filiform bougie, I think the results have been *simply marvellous*. And no matter what is said or

who disputes the efficacy of electrolysis in urethral strictures, I will swear by it every time; for facts are stubborn things that cannot be ignored, and have been proved beyond the shadow of a doubt, *under my own observation and in my own person*. I am satisfied that, to accomplish the best results from electrolysis in urethral stricture, the seances ought not to be very close together. I should say two weeks, unless circumstances were such that the patient could not be gotten at at pleasure."

It will be seen that I withhold the gentleman's name, but the case can be vouched for by Prof. Shoemaker, of Philadelphia, and if Dr. Keyes is still *investigating*, and would like to see a Simon pure case in the person of a most accomplished physician of large experience, and wants to get at bottom facts, I shall be only too glad to furnish the name and address to him.

W. C. WILE, M. D.

Danbury, Conn., Nov. 26, 1888.

An abundance of more evidence is on file.

68 W. 36th st., New York, Dec., 1888.